

Where Does the Political Budget Cycle Really Come From?

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ABSTRACT: Whereas a political budget cycle was once thought to be a phenomenon of less developed economies, some recent studies find such a cycle in a large cross-section of both developed and developing countries. We find that this result is driven by the experience of “new democracies”, where fiscal manipulation may be effective because of lack of experience with electoral politics or lack of information that voters in more established democracies use. The strong budget cycle in those countries accounts for the finding of a budget cycle in larger samples that include these countries. Once these countries are removed from the larger sample, the political budget cycle disappears. Our findings may reconcile two contradictory views of pre-electoral manipulation, one arguing it is a useful instrument to gain voter support and a widespread empirical phenomenon, the other arguing that voters punish rather than reward fiscal manipulation.

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1. Introduction

The common perception is that incumbents often try to use expansionary economic policy before elections to increase their re-election chances. Most politicians and non-politicians alike would probably subscribe to this view, and the term “election-year economics” or its equivalent is common in many countries.¹

In the political economy literature, this view is summarized as the “political business cycle”, that is, the possibility of a macroeconomic cycle induced by the political cycle. Models of the political business cycle are motivated by the finding that good macroeconomic conditions prior to the elections help an incumbent to get re-elected, a finding that has wide support in studies (conducted mainly in developed economies).² The strength of this finding was an important factor generating formal modeling of how opportunistic incumbents may manipulate economic policy to induce economic expansions before elections.

However, notwithstanding both common perceptions and the substantial evidence that a “strong economy” helps incumbents get re-elected, empirical studies – especially in developed economies – provide little evidence of a regular and statistically significant increase in economic activity before elections.³ In short, voters care about the economy but this does not appear to translate into econometrically verifiable cycles in aggregate economic activity.

Given the lack of evidence for political cycles in economic outcomes, a literature examining possible cycles in policy instruments has developed. More specifically,

¹ Tufté (1978, p.3) begins his famous book on the political business cycle with a quote from 1814, “A Government is not supported a hundredth part so much by the constant, uniform, quiet prosperity of the country as by those damned spurts which Pitt used to have just in the nick of time.”

² The most influential work was probably that of Fair (1978) (updated in Fair [1982, 1988]), who found similar results for the U.S. In his original article, Fair looked at presidential elections from 1916 through 1976, and found that the change in real economic activity in the year of the election appears to have an important effect on votes for president. Specifically, a one percent increase in the growth rate increases the incumbent’s vote total by about one percent. Numerous other articles find similar results on the importance of pre-election conditions on voting patterns in both the U.S. and other countries. Looking at voting or popularity functions, Lewis-Beck (1988) found that the sort of results that Fair reports for the U.S. hold in Britain, France, West Germany, Italy and Spain as well. Madsen (1980) reported similar results for Denmark, Norway, and Sweden.

³ See Drazen (2000), chapter 7, for a review of the empirical evidence on opportunistic political business cycles in economic activity.

attention has turned more to examining the existence of fiscal expansions in election years, meant to generate the desired economic and hence electoral effects. A political fiscal cycle (or, political budget cycle, to use Rogoff's term) may be consistent with the lack of evidence on a political cycle in economic activity if, for example, fiscal policy is targeted to specific groups of voters. That is, it is not macroeconomic expansion that politicians are after, but influence on specific constituencies. Fiscal expansions are thus a reflection of targeted expenditures and tax cuts used to draw support, rather than the result of an attempt to increase aggregate economic activity. Moreover, there is a "revealed preference" argument. If politicians choose expansionary policies before elections, it is likely that they "get" something out of it, namely, they increase their chances for reelection, even though the econometrician is unable to observe any effects on aggregate economic activity.

The conventional wisdom is that the political budget cycle indeed exists, reflecting the desire of incumbents to get re-elected. Until recently, the political budget cycle was thought to be a phenomenon primarily of developing rather than developed countries, where examples of a political cycle in aggregate fiscal variables were less common.

A number of recent papers using large cross-country data sets have argued that the political cycle in fiscal aggregates is found in *both* developed and less developed countries. Shi and Svensson (2002b) find evidence of significant pre-electoral decreases in the fiscal balance (*i.e.*, increases in the government budget deficit) in a panel of 91 developing and developed countries over the period 1975-95. (See also Shi and Svensson [2002a].) Persson and Tabellini (2002) in a sample of 60 democracies over the period 1960-98 find no evidence of a statistically significant pre-electoral deterioration in the overall fiscal balance, but do find evidence of statistically significant tax decreases before elections. (See also Persson and Tabellini [2003, chapter 8].) The apparent strength of the results has fostered the view that the political fiscal cycle is in fact a widespread phenomenon.

A very different view suggests that politicians have very limited ability to successfully manipulate the economy to help their re-election chances, thus casting doubt on the widespread existence of macroeconomic political budget cycles. Proponents of

this alternative view accept the positive effect of a “strong economy” on an incumbent's re-election prospects. However, such an effect does not automatically imply that opportunistic politicians can successfully engage in “election-year economics” at the aggregate level. There are at least two reasons to question whether politicians will engage in pre-electoral monetary and fiscal expansion in order to manipulate aggregate economic activity. First, there is the technical question of whether it is possible to time the expansion accurately enough to happen just before the elections. Though high precision missiles may now dominate military conflict, the economic equivalent in electoral conflict is believed not to exist. It is impossible to fine-tune the aggregate economic effects of economic policy so that they can be turned on and off with any precision.⁴

Even if it were technically possible to precisely time the aggregate effects of policy, there is another key reason why politicians may not do so. Policies that shift the timing of economic activity so that the economy expands before an election are considered harmful to the economy over time in terms of “unsmoothing” consumption, inducing investment cycles, *etc.* Clearly, if voters are rational they would not support such policies, so that pre-electoral manipulation would be punished rather than rewarded at the polls, as is argued by a number of studies such as Peltzman (1992), Alesina, Perotti and Tavares (1998), and Brender (2003). These studies present evidence that voters in developed economies are “fiscal conservatives” and often tend to remove deficit-producing incumbents from office. Brender – and others – also discuss the conditions under which voters would punish deficit producing politicians, pointing to the importance of the availability of information – including the existence of media that would deliver the information to voters. The more available information is, the more likely it is that voters would punish fiscal manipulation.

In short, an incumbent might be rewarded at the polls only if he can hide the manipulation and make the public believe that the good economic conditions reflect the success of his policy or his high ability. However, this assumption seems unreasonable in

⁴ Lewis-Beck (1988) argues that the absence of a significant opportunistic cycle either in outcomes or in instruments reflects how hard it is to time economic manipulation. Since monetary and fiscal policy can be used only with great imprecision, so that politicians cannot expect to time the stimulus to come right before an election, opportunistic politicians will try to provide for continual good economic news.

many countries because voters – *especially experienced ones* (who understand the incentives and the tools of electoral manipulation) – know that election years are particularly “suspect” for manipulation and therefore would interpret “surprises” in these years with special caution. Therefore, in economies in which the electorate has had a lot of experience with elections, and where the collection and reporting of the relevant data to evaluate economic policy are common, voters would be unlikely to “fall” for the trick of making the economy look good right before elections. It is therefore perhaps not surprising that political cycles in aggregate activity are not as easy to find as one might initially think. This may be because politicians who try to influence economic activity are simply unsuccessful in doing so or because they realize that manipulation may be seen as such and therefore would not help their re-election chances.

Fiscal manipulation may occur at a level other than the aggregate, for example, transfers to one group offset by a reduction in transfers to other groups of voters or in changes in the composition of spending towards spending valued by “impressionable” voters. This would be consistent with voters being fiscal conservatives who dislike the need to finance higher aggregate spending, rather than disliking electoral economics *per se*. It would also be consistent with it being harder to detect fiscal manipulation that doesn't affect the overall size of the budget, especially, as we argue, in “new democracies”.⁵ We stress that our interest is in testing for the existence of political cycles in the aggregate fiscal data.

In this paper we re-examine recent empirical results on the existence of the political budget cycle in a cross-section of countries. While we also find a political cycle in the fiscal balance in a large cross-section data set, we argue that this finding is driven by the experience of “new democracies”, where fiscal manipulation may work because voters are inexperienced with electoral politics or may simply lack the information that is produced in more established democracies. It is the strong fiscal cycle in these countries that accounts for the finding of a fiscal cycle in larger samples including these countries. Once these countries are removed from the larger sample, any political fiscal cycle disappears. Our findings also reconcile two contradictory views of pre-electoral

⁵ Drazen and Eslava (2003) present evidence on the importance of composition of spending effects for the political budget cycle in Colombia.

manipulation, one arguing that it is reasonable to expect politicians to engage in such manipulations and that empirically they are widespread, the other arguing that voters punish rather than reward fiscal manipulation.

The plan of the paper is as follows. In the next section we summarize the existing evidence for a political budget cycle. This includes earlier evidence for the political fiscal cycle, mostly from developing countries, as well as more recent papers arguing that a cycle is observed in both developing *and* developed countries. In section 3, we set up the basic empirical work, discuss a number of data and estimation issues, and present the basic regressions for the set of democracies as a whole. In section 4, the heart of the paper, we demonstrate that the political budget cycle found in larger data sets is due to the significant political cycle in “new democracies”. In section 5, we suggest some conceptual bases for the result that the political budget cycle is a phenomenon of “new democracies”. Section 6 concludes. A Data Appendix contains a detailed description of the data.

2. Evidence on Fiscal Cycles – A Summary

Until recently, conventional wisdom was that a political fiscal cycle was more a phenomenon of developing rather than developed economies. In this section we review the empirical evidence.

For developing countries, there are a large number of both country and cross-country studies. Ames (1987) presents a panel study of 17 Latin American countries in which he shows that over the period 1947-1982, government expenditures increased by 6.3% in the pre-election year and decreased by 7.6% in the year after the election. Block (2000) presents evidence of a political business cycle in both fiscal and monetary policy in a cross-section of 44 Sub-Saharan African countries. Schuknecht (1996) is a comprehensive study of the political business cycle in 35 developing countries over the period 1970-92.⁶ He argues that there should be more room for manipulation in developing countries, as checks and balances are weaker and the incumbent has more power over monetary and fiscal policy. He suggests that in developing countries

⁶ See also Block (2002) for a recent cross-section study.

expenditure policies, such as distribution of free or subsidized goods or employment generation via public works programs, are probably more effective than tax cuts to affect voter behavior. He finds a clear significant effect of elections on the fiscal balance, but no significant effect on output. Individual country studies arguing for a significant political fiscal cycle include Ben-Porath (1975) for Israel over the period 1952-73, Krueger and Turan (1993) for Turkey over the period 1950-1980, and González (2002) for Mexico over the period 1958-1997, to name a few. Drazen (2001) presents further discussion.

For developed countries, fiscal manipulation observable at the aggregate level is thought to be less common. Alesina, Cohen, and Roubini (1992) find a budget balance cycle in a set of 13 OECD economies in an unbalanced panel over the period 1960-1993 (about half of the countries have observations only from 1970 onward), but no significant cycle in the components of the budget. In the United States, Keech and Pak (1989) found a cycle for veteran benefits in the United States between 1961 and 1978. Alesina, Cohen, and Roubini (1992) find evidence of a political cycle in transfers relative to GNP in the U.S. over 1961 to 1985, which they argue disappears if one extends the sample either forward or backward. They find no statistically significant political cycle in other fiscal instruments.

Two recent studies challenge the view that the political fiscal cycle is primarily a phenomenon of developing countries and that evidence from developed economies is mixed at best. Shi and Svensson (2002b) consider a panel data set of 91 countries, both democracies and non-democracies, over the period 1975-95. They find that, in an election year, the government surplus falls significantly in both developing and developed countries. Both government spending rises and revenues fall, though the significance differs across the data sets and the estimation technique. The economic effect is significant for the sample as a whole, the fiscal surplus falling on average in their full sample by 1 percent in an election year.

Persson and Tabellini (2002) argue that there is a strong political budget cycle in developed economies as well. They restrict the sample to countries with democratic political institutions and competitive elections and consider a group of sixty democracies from 1960 to 1998. They find a political revenue cycle (government revenues as a

percent of GDP decrease before elections), but no political cycle in expenditures, transfers, or the overall budget balance across countries or political systems.

3. Estimating Political Budget Cycles in Democracies

The work of Shi and Svensson and of Persson and Tabellini presents a serious challenge to the previous conventional wisdom that the political budget cycle better characterizes less-developed economies than developed economies. In this section, we consider some empirical issues and examine the basic results. As is well known, since the IFS data on which many studies are based are noisy, the data need to be “cleaned”. We set out in Table A1 in the Appendix, on a country-by-country basis, what are the problems with the data and what were the adjustments that we made. (The data are available at <http://www.tau.ac.il/~drazen>.) On this basis, we then estimate similar equations, using the same economic controls, variable definitions, and samples. Our main conclusion is that in a broad cross-section of democracies over the period 1960-2001 there indeed exists a political cycle in the fiscal balance, though the strength of the cycle is sensitive to variable definitions, the time period, or the set of countries included. In section 4 we will refine this further, and show that the crucial country characteristic is whether the country is a “new” or an established democracy.

Our basic data set consists of 107 countries for which we collect data on the central government balance, total expenditure and total revenue and grants from the IFS database. (Further details are given in the Data Appendix and Tables A1 and A2.) The sample period is 1960-2001, although the data for many countries cover shorter periods.

Many cross-section studies of the political fiscal cycle, like Shi and Svensson, do not restrict the data to include only elections that take place in democracies. In our view, if the political budget cycle reflects the manipulation of fiscal policy to improve an incumbent’s re-election chances, then it only makes sense in countries in which elections are competitive. If elections are not competitive, then the basic argument underlying the existence of a political budget cycle loses much of its validity. Even if one finds a “political” budget cycle in countries that have no competitive elections (such as Romania before 1990, Indonesia between 1975 and 1995 or Syria, to name a few examples from this study), the explanation cannot be based on the desire of an incumbent to improve his

re-election chances.⁷ In fact, one might argue that finding a political budget cycle in non-democratic countries weakens the support for the theory, rather than strengthening it. Hence, from either an empirical or conceptual perspective, one needs to separate democratic from non-democratic countries.⁸

We therefore separate democracies from non-democracies, analogous to Persson and Tabellini, by applying to these data a filter for the level of democracy in each country in each year. This filter is taken from the POLITY IV project, conducted at the University of Maryland, covering nations with a population exceeding half a million people. Each country is assigned in this dataset a value that ranges from -10 (autocracy) to 10 (the highest level of democracy). We restrict our sample to democracies, by selecting only the countries that receive a score between 0 and 10 on this scale; this reduces our sample to 69 countries. These countries may be classified as those that were in the OECD for the entire sample period, the “transition” economies of Eastern Europe and the former Soviet Union (for the period 1990-2001), and all others. Tables A1 and A2 provide a list, as well as a description of the available data for each country.⁹@

Election dates and institutional data on the election process are taken from the DPI dataset, provided by the World Bank (Beck et. al., 2001). These data were complemented, where needed, by other political datasets, such as the IDEA (Institute for Democracy and Electoral Assistance, “Voter Turnout Since 1945 to Date”) and IFES (International Foundation for Electoral Systems, election guide).

The basic regression is of the form:

$$f_{i,t} = \sum_k b_k f_{i,t-k} + \sum \mathbf{c}' \mathbf{x}_{i,t} + dELEC_t + \mu_i + \varepsilon_{i,t} \quad (1)$$

⁷ There may of course be cases where dictators may want to eliminate any possible signs of discontent before “sham” elections, but this is neither the rationale for observing a political budget cycle nor would it be a convincing empirical regularity. Alternative explanations of pre-election fiscal expansions that might be observed under both competitive and non-competitive electoral systems would include multi-year economic plans which coincide with the term of governments or “end of term” budgeting effects.

⁸ It is too simple to argue that including non-democratic countries in the sample simply lowers the probability of finding significant results. The model should be tested separately for democracies and other countries.

⁹ Table A3 lists countries that were excluded, either because: IFS data doesn't exist, even though some other studies include these countries; IFS data exist, but they were not democracies; or, because, though the country is democratic, we judged the IFS data to be of very low quality.

where $f_{i,t}$ is a fiscal indicator in country i in year t , $\mathbf{x}_{i,t}$ is a vector of control variables, $ELEC_t$ is an electoral dummy, and μ_i is a country fixed effect. (Year effects were generally insignificant and were dropped from the regressions.¹⁰) In the tables, we present only the coefficient of the electoral variable, indicating whether or not there is a statistically significant political cycle.

Our control variables include those used by Shi and Svensson and by Persson and Tabellini. In addition to fixed country and year effects the former include, the log of real GDP per capita (taken from the 2002 version of the World Bank's World development Indicators dataset (WDI)) and the growth rate of real GDP. The latter include real GDP per capita, the trade share, two demographic variables representing the fraction of the population aged 15-64 and 65+ (also taken from WDI). We also include, as do Persson and Tabellini, the log difference between real GDP and its (country specific) trend (computed using the Hodrick-Prescott filter), as a measure of the output gap.

The electoral dummy, which is that used by these authors, is meant to capture pre-electoral effects, and equals 1 in an election year and 0 otherwise, no matter when during the year the election occurred. We denote it by ELECTWB, as it is based on the DPI project conducted at the World Bank.¹¹

Using country fixed effects in a regression with lagged dependent variables introduces a potential estimation bias that is of order $1/T$, where T is the length of the panel, even as the number of countries becomes large. (See, for example, Nickell [1981] or Wooldridge [2002].) The bias arises because the initial condition $f_{i,0}$ is correlated with the country fixed effect μ_i , so that the lagged dependent variable is correlated with the error term. This problem is thought to be especially severe in micro panel data, where

¹⁰ The insignificance of the year effects may be due to the inclusion of controls for the level of economic activity in each country in each year.

¹¹ Endogeneity of election dates in parliamentary regimes to the level of economic activity could in theory produce an endogeneity bias, as our fiscal variables are scaled by GDP. The use of a control for the level of economic activity relative to trend should help rule out simultaneity bias from the error term being correlated with the election date.

the number of individuals i is large, while T is quite small, often less than half-a-dozen. (One alternative is a GMM estimator, which also exhibits a small sample bias.¹²)

Since the bias of the fixed effects estimator is of order $1/T$, the magnitude of the bias in our estimates reported below depends on which sample and fiscal indicator we use. In a panel of developed countries from 1960-2001, the average length of the sample is 33 years when we consider the fiscal balance and 35 years when we exclude new democracies. (Remember that some countries do not have data for the entire period.) Hence the bias in these estimates is likely to be small. Our panel of “old” democracies in the whole sample, where we find no fiscal cycle (Table 5), is of a similar length. Our panel of elections in “new democracies” in the sample as a whole, where we find a strong cycle (Table 3), is considerably shorter (12 years including transition economies, 13 years excluding them), so that the potential bias may be greater. That is why we present various tests of the “new democracy” effect which rely on longer time series.

In Table 1, we present regressions for the fiscal balance, revenues and expenditures, all as a percentage of GDP, similar to those presented by Shi and Svensson and Persson and Tabellini, that is, using the same controls, variable definitions, and sample periods over a very similar set of countries in each case. In equation set 1 in the table, we reproduce the basic Shi-Svensson regressions over the same time-period 1975-95. There is a highly significant political cycle in the fiscal balance that reflects a decline in revenue in election years.¹³ The coefficients that we estimate for both the fiscal balance and revenues are very close to those Shi and Svensson found for a fixed-effects estimator. In equation set 2, we consider the same regressions over the same time period, but where we restrict the sample to democracies by using the POLITY filter. We find that the fiscal balance cycle is still highly significant.¹⁴

We also find a significant political revenue cycle over the time period 1975-95. However, this cycle is due to one country, Sweden, in which there was a “jump” in the

¹² Wooldridge (2002) presents a discussion of the advantages and disadvantages of the two methods.

¹³ This political revenue cycle is *not* significant in a sample of *only* non-democratic countries over 1975-95. We also find that the significance of the revenue cycle is very sensitive to the choice of time period. (Results available on request)

¹⁴ The qualitative results in these and all other regressions are not significantly affected when the standard errors are calculated using the White Heteroskedasticity Consistent Covariance correction. (Results available on request.)

revenue and expenditure series in the early 1990s in the IFS due to a transfer of functions from central to local governments.¹⁵ Once the regressions are estimated with Sweden excluded, the significant revenue cycle disappears. The same result arises in samples of old democracies or in OECD countries. In each case, samples including Sweden showing a significant revenue cycle, which disappears when Sweden is excluded.

In equation sets 3 and 4 we extend the sample period to 1960-2001 in a sample only of democracies, and find a significant fiscal balance cycle, though with a smaller coefficient. There is no political cycle in either revenues or expenditure. In equation set 5 we consider a similar exercise for the results of Persson and Tabellini (2002), who restrict the sample to democracies and use a different set of control variables, as discussed above. We use the sample period 1960-2001, analogous to their sample period. As above, we find a statistically significant cycle in the fiscal balance, but no political cycle in revenues or expenditure.¹⁶

To summarize our results so far, over the time period of our sample, 1960-2001, there is a statistically significant political cycle in the government budget balance using either the Shi-Svensson or Persson-Tabellini controls. In an election year, the deficit rises by about three-tenths of one percent of GDP relative to non-election years. For reasons we discuss in the next section, we believe that the significant results that are found are driven by a subset of countries and electoral incidents.

4. The Empirical Importance of Being a New Democracy

A key point of the previous section is that it is important to distinguish between democracies and non-democracies. Conceptually, it should matter whether or not elections are competitive. Empirically, we found some evidence that it does matter in comparing results for a panel of only democracies to one that includes both types of countries.

¹⁵ For example, reported central government expenditures dropped from 51.7 percent of GDP to 34.6 percent of GDP from 1993 to 1994, an election year, and reported revenues dropped from 37.7 to 26.3 percent of GDP from 1993 to 1994.

¹⁶ The difference in significance levels may reflect the much lower standard deviation of the dependent variable, less than 4 for the fiscal balance, greater than 10 for expenditures, greater than 11 for revenues.

Using a political filter to refine the data set brings out another interesting phenomenon – the number of countries in the sample is increasing over time in both the sample as a whole and in the subsamples of developed and developing countries. In the 1960s there 31 democracies in the sample as a whole. This rises to 44 in the 1970s, 53 in the 1980s, and 59 in the 1990s, not counting the formerly socialist economies. If the transition economies are included the number of democracies rises to 69 in the 1990s, more than twice the number in the 1960s. In short, when a filter is used to select democracies, the data set changes significantly as the time period changes.

More specifically, *new* democracies are being added to each of the samples over time, both for developing *and* developed economies. Hence, one is lead to ask whether it is the additional countries, where democratic elections are a new phenomenon, that are responsible for the political fiscal cycle found in the panels?

There are good ex-ante reasons to think so. Many models arguing that voters hold incumbents accountable for deficits and wasteful spending would predict that incumbents who value office would cut rather than increase spending, especially in developed economies, where government expenditure is high relative to GDP. (See Peltzman (1992) Besley and Case (1995), Alesina, Perotti and Tavares (1998), among others.) For this to be the case, one would require, however, that voters have both the necessary information to draw such inferences, as well as the ability to process that information correctly. These would reflect experience with an electoral system by voters, the establishment of the institutions that would collect and provide the relevant data, and experience by media in disseminating and analyzing this information. In the absence of this experience, it is more likely that fiscal manipulation would be rewarded rather than punished, so that incumbents would successfully engage in it. We will return to these arguments in more detail in section 5 below.

Another reason why the interpretation of economic data by voters may be more complicated in new democracies is the shift in economic structure that often goes along with the shift to democracy. This is perhaps most striking when one considers the formerly socialist economies in Eastern Europe and the former Soviet Union where the centrally planned economic system and the reporting mechanisms were abolished in a relatively short period. The collapse of old economic systems may also present a

problem in the analysis of the political fiscal cycle in these countries: to the extent that high deficits associated with the economic transition occur simultaneously with the political transition, without either one causing the other, one would not classify this as a classic political fiscal cycle. On the other hand, politicians facing the new phenomenon of contested elections who are aware of the desire for rapid economic transition may respond especially strongly with deficit spending.¹⁷

To test this hypothesis, we separate “new democracies” from established democracies in our sample. Beginning with the POLITY filter, we separate those countries that had competitive elections during the entire sample period for which we have data from those that began having competitive elections only within the sample period. For the latter, we take observations for the first four competitive elections and define those observations as coming from a “new democracy”. In the data Appendix, we list those observations characterized as “new democracies” in both the sample of developed and less developed countries.

A. OECD Countries

We first present the results for countries that were members of the OECD for the entire sample period, roughly corresponding to a set of developed countries. There are four “new democracies” in the sample period in this group – Greece, Portugal, Spain, and Turkey. While there are not enough data points to test for a political fiscal cycle in a sample of only new democracies, we can estimate the equations both with and without these four countries. In Table 2, we present results for the political fiscal cycle in OECD countries using Persson and Tabellini's control variables (equation sets 1 and 2) and Shi and Svensson's control variables (equation sets 3 and 4). What we see quite clearly is that once the new democracies are removed from the sample, so that the sample contains only established democracies, the fiscal balance cycle found in the group of OECD countries as a whole disappears. Given the average length of the panel, approximately 33 years (35 excluding the new democracies), the bias from including a country specific fixed effect in the presence of dependent variables is probably negligible.

¹⁷ This suggests that one needs to be careful in how one treats the transition economies in the first years after transition, and in how one interprets the results of any study that simply lumps them together with other countries. To err on the safe side, we exclude all the elections that took place in the first two years following the transition.

Though there is no significant cycle in the fiscal balance in a sample of established democracies, the regressions reveal a statistically significant revenue cycle in the subset of established democracies. (This is similar to the pre-election revenue cycle that Persson and Tabellini find.) As in the case of the set of democracies as a whole discussed at the end of section 3, this cycle is due to the Swedish data. When the regressions are estimated without Sweden, the significant revenue cycle disappears.

Hence, one may conclude that the political fiscal balance cycle in new democracies is driving the results for the sample of OECD countries as a whole. Put another way, when we look at a constant panel of democracies among OECD countries over the whole sample period, there is no statistically significant political budget cycle. This is consistent with the conventional wisdom prior to the work of Persson and Tabellini and Shi and Svensson, as well as with the literature that casts doubt on the existence of fiscal manipulation in countries with electoral experience.

B. New Versus Old Democracies in the Sample as a Whole

We now consider the sample of both developed and developing countries as a whole, distinguishing new from established democracies (For a list of the “new democracies” see Table A2). In Table 3 we present results over only new democracies in the sample both including and excluding the new democracies in Eastern Europe, for the entire sample period. We present regression results using both the Persson-Tabellini controls and the Shi-Svensson controls. A number of results stand out. First, we get a significant fiscal balance cycle for the set of new democracies, whether or not the formerly socialist economies are included. The coefficients on the electoral variable are larger than in the sample of all democracies. We also find, in contrast to all other results presented so far, that there is a significant political expenditure cycle in the new democracies (as suggested, for example, by Schuknecht [1996]) if the formerly socialist economies are included. Moreover, note that the coefficients on the fiscal balance and on expenditures in the analogous equations are very similar, while the coefficient on revenues is smaller in absolute value and not significantly different from zero. When the formerly socialist economies are excluded, however, the expenditure cycle disappears, though the fiscal balance cycle remains significant.

To further test the “new democracy” effect, we run regressions for the sample as a whole, that is both new and old democracies, including separate dummy variables for each of the first four elections, and a dummy for all elections after the fourth (including elections in “formerly” new democracies). The results are presented in Table 4, using both the Persson-Tabellini controls and the Shi-Svensson controls. Each of the four new election dummies is significant in regressions for a fiscal balance cycle, with approximately equal magnitude, while the coefficient on the dummy for elections after the fourth is not significant. Moreover, starting with the second election, the significance of the coefficient is dropping as one moves to the third and fourth elections, suggesting that electoral fiscal effects may be becoming less strong in new democracies as there is more experience with elections.¹⁸ Analogous to our other results there is no significant political cycle in revenues or expenditures when separate election dummies are used.

Another way to test the hypothesis is to look at the counterpart sample of “old” democracies (that is, all countries which were in a sample of democracies using the POLITY filter, *excluding* the new democracies). To confirm that the cycle is indeed a phenomenon of new democracies, in Table 5 we present results on the existence of a cycle in the fiscal balance and in expenditures in the established or “old” democracies. As our hypothesis suggests, we find no statistically significant political cycle in this set of countries. This is true no matter what controls or electoral dummy we use.¹⁹ The same is true when we look only at developing country old democracies.

¹⁸ The lower significance level of the first election dummy relative to the second may represent a number of things. The fiscal observations for the first election may be noisier since many other things are going on in the first years of a new democracy. It may be that fiscal manipulation is really stronger in the second election, as incumbents face re-election for the first time. Finally, there are many fewer observations for first elections, as the lag structure implies that many of these observations drop out.

¹⁹ There are two ways one may exclude elections in “new democracies” in testing for a political cycle in “old” democracies. One is to exclude all elections (*i.e.*, all observations) that is, to exclude those countries that made the transition to democracy in the sample period entirely. The other is to exclude only those election observations which occurred when the democracy was in fact “new” (the first three or four elections after the transition to democracy in our definition), but to include all other observations for these countries in a sample of elections in established democracies. As we cannot be sure *a priori* how long the new democratic effect persists (we take four elections as a possible minimum), we prefer the first procedure and present results using that procedure. We ran the regressions using the second definition of “old” democracies and found the same results.

C. Alternative Hypotheses

Persson and Tabellini suggested that the nature of the political cycle depended on the nature of electoral rules – whether a country had a parliamentary or presidential system and whether voting for the legislature was primarily via proportional or majoritarian rules. In Table 6, we consider the first distinction and find that after controlling for electoral rules, the political fiscal cycle is still a phenomenon of new democracies. We split our electoral dummy into two: one for elections in parliamentary systems, the other for elections in presidential systems. In equation set 1 in the table, we show that there is a significant fiscal balance cycle in both types of systems when we consider the entire set of democracies over the whole sample period. (The statistically significant revenue cycle in parliamentary democracies disappears once Sweden is excluded, as discussed at the end of section 3 above.) In equation set 2 we consider only new democracies and find the same significant fiscal balance cycle in both parliamentary and presidential systems. As equation set 3 shows, there is no analogous political fiscal balance cycle in either parliamentary and presidential systems when only old democracies are considered. (As in the sample of all democracies, the significant revenue cycle in parliamentary democracies disappears once Sweden is excluded from the set of old democracies.) Hence, if one distinguishes between parliamentary and presidential systems, the political cycle is still found to be a phenomenon of new democracies, where it exists regardless of the electoral system.

Another alternative hypothesis is that it is not the length of time a country has been a democracy, but the level of democracy that matters for the existence of a political fiscal cycle. That is, the political fiscal cycle may be a phenomenon of countries where democracy is relatively weaker. (See, for example, Shi and Svensson [2002a] and González [2002].) In Table 7a we show that indeed the political fiscal cycle is stronger in countries with lower level (“quality”) of democracy. The fiscal balance cycle is significant in those countries where the POLITY index of democracy is between 0 and 9, whereas it is insignificant in countries with a POLITY index of 10. However, once we separate old democracies from new democracies we find that the apparent effect of the level of democracy is entirely due to the new democracies. In Table 7b we show that for new democracies, the fiscal balance cycle is significant, regardless of the level of

democracy. In Table 7c we show that for old democracies there is no political budget cycle, whatever their level of democracy is. (The significant revenue cycle for old democracies with a POLITY index of 10 disappears once Sweden is excluded.) There is a significant political expenditure cycle for new democracies with a POLITY index of 10, though none for those with a POLITY index of 0-9. Consistent with our earlier results for transition economies as a whole discussed in section 4B, the significant cycle in the new democracies with POLITY = 10 may reflect the transition economies in that group.

The reason that we find stronger evidence for political fiscal cycles in countries with a lower level of democracy is probably that the proportion of new democracies in this group is higher: 50 percent of the data points in the group of countries with a low level of democracy are new democracies, compared to 7 percent among the countries with a high level. The findings in Tables 7a-7c also rule out the explanation that the results for new democracies actually reflect their lower level of democracy, rather than their being “new”.

To summarize, our empirical results are quite clear. In terms of a *group* of countries, the political budget cycle is a phenomenon of new democracies. It is not a widespread phenomenon of democracies as a whole, as some recent research has argued. The finding that the political cycle is a widespread phenomenon across a larger group of countries comes from failing to distinguish new democracies from other countries, namely established democracies and non-democracies.

We should stress that we are not arguing that fiscal manipulation does not occur at all in other countries, only that it is not sufficiently prevalent and large to show up as an econometrically significant regularity in the aggregate fiscal data for groups of countries other than new democracies. Of course, there may be incidents of aggregate fiscal cycles in other countries, as well as fiscal manipulation that is not observable in the aggregate fiscal data, as argued in the introduction to the paper. But, in terms of a group of countries, it is the new democracies where the political fiscal cycle is really occurring.

5. The New Democracy Effect – Some Conceptual Observations

Why are new democracies more susceptible to election-year economics? It is beyond the scope of this paper to investigate this question in any depth, though we hope

to do so in the future. Here we do three things. First, we suggest reasons why it may be the case that new democracies are more likely to display political fiscal cycles. Second, we briefly discuss how this may be modeled. Third, we point out how our findings help to reconcile the logic behind the political fiscal cycle (that is, expansionary fiscal policy may be used to try to increase electoral prospects) with the view that argues that pre-electoral expansions are punished rather than rewarded.

Why might electoral cycles be more likely in new democracies? We argued above that for voters to hold incumbents accountable for deficits (and hence for deficits to be punished rather than rewarded at the polls) they need both the necessary information to draw such inferences, as well as the ability to process that information correctly. These would reflect experience with an electoral system, the availability of data, and the experience of the media in finding, disseminating and analyzing the relevant data. In many new democracies, even basics like the collection of data and reporting it to the public are not well established, so that fiscal manipulation is easier to do. (The demand for data may in fact be driven in part by the possibility of holding office-holders accountable through elections.)

Another reason why electoral cycles may be more likely in new democracies concerns the potential difficulties in identifying the “pivotal” voter in these situations. When competitive elections are a new phenomenon, politicians are unsure who are the pivotal voters, so that transfers meant to woo voters must be spread more widely.²⁰

We want to stress that the ability to draw inferences about incumbent performance from pre-electoral economic variables is not meant simply to represent the experience of voters, but of experience and interactions of all actors with the electoral system. Put another way, it is not that new democracies are characterized by unsophisticated or naïve voting population, but that in countries with less of an electoral history, and hence less exposure to pre-electoral fiscal manipulations, a political cycle is more likely to occur. Our results suggest that learning about pre-electoral fiscal manipulations is a local learning process that is probably not easily transferable across countries.

How might one model the process of gaining experience with fiscal manipulation in order to gain insight into the new democracy phenomenon? This is still work to be

²⁰ We are indebted to Alessandra Casella for this suggestion.

done. The observations about the inference problem in the previous paragraph suggest using some sort of a signaling model of candidate characteristics under imperfect information, as in the Rogoff (1990) model of political fiscal cycles driven by rational voters with imperfect information. Imperfectly informed rational voters respond positively to pre-electoral fiscal expansions because they signal a characteristic valued by voters. González (1999) and Shi and Svensson (2002a) extend Rogoff's model to study the effect of the degree of democracy and the level of institutions on the magnitude of fiscal cycles. Both models stress the importance of "transparency," meaning the probability that voters learn the incumbent's characteristics costlessly, that is, independent of signaling. The higher the degree of transparency, the smaller is the political budget cycle.

In the Shi and Svensson (2002a) model a fraction of voters are assumed to be "informed", that is, have access to a free flow of information, so that fiscal expansions provide them with no information. In contrast, uninformed voters use fiscal expansions to try to infer candidate characteristics, with such expansions making it more likely they vote for the incumbent. As the fraction of informed voters rises, the magnitude of the political budget cycle decreases. They argue that better access to the media combined with the skill to process information provided may help voters overcome the lack of information that is key to successful fiscal manipulation, but both are likely to be distributed unequally across the population.

Our argument presented above is very similar in some ways to this argument, whereby greater availability of information and a better ability to process it reduces the scope for or altogether eliminates the possibility of fiscal manipulation by politicians. An essential difference is that whereas Shi and Svensson (2002a) (and González) view transparency primarily as a characteristic of political systems (that may evolve over time, with institutional change or development), our new democracy results suggest a somewhat different view. "Transparency" reflects experience with the elections themselves, with the crucial variable being the length of time a country has been a democracy, rather than the level of democracy. Our findings in Table 7, namely that results on the importance of the level of democracy may actually reflect the new democracy effect, suggest the importance of distinguishing the two. A key implication of

our view is that the signal content of fiscal actions necessarily *change* over time as voters became more experienced over time with electoral fiscal manipulation and were provided with more economic and fiscal information in order to draw inferences. Hence, any positive effect of deficit spending on an incumbent's electoral prospects would not only diminish over time, but would likely change sign as a country has more experience with a competitive electoral process.

This last point brings us back to the relation between the theory of the opportunistic political business cycle, predicated on the view voters may reward deficit spending at the polls, and the view that voters may punish deficit spending at the polls. Our results for new democracies are consistent with the first view, while the findings for established democracies are consistent with the second. Proponents of the latter view, such as Peltzman (1992) or Alesina, Perotti, and Tavares (1998) looked at established democracies, and it is not surprising that they do not find support for an electoral benefit of deficit spending. Our new democracy result, and the view that there is a learning process which leads to the empirical disappearance of a political fiscal cycle can reconcile and make consistent these two approaches. The results of Brender (2003), showing how the electoral response to deficit spending in local Israeli elections changed dramatically over time, are especially enlightening in this regard. In that study it was shown that when direct elections for mayors were introduced in Israel, voters were indifferent to deficits and local fiscal management for 15 years. However, after that period, when accounting and reporting standards were enforced on the local authorities, and when the local media expanded, deficit spending was “punished” at the polls.

6. Conclusions

In this paper, we considered the empirical evidence for the existence of a political budget cycle. The question of *whether* such a cycle exists on the macroeconomic level across countries is really a question of *where* it exists, that is, in which types of countries. The answer to that question is not only empirically relevant, but theoretically important as well, since it sheds light on what factors lie behind the existence of a cycle.

It was once thought that the political budget cycle was a phenomenon of developing economies, suggesting that it might reflect low levels of democracy or of

economic development. This was consistent with research that argued that experienced voters punish electoral fiscal manipulation. Some recent research finds such a cycle in both developed and developing countries, calling into question this conceptual interpretation of the existence of a political fiscal cycle.

Our empirical results indicate quite clearly that the political fiscal cycle is a phenomenon of new democracies. The strong fiscal cycle in those countries accounts for the finding of a fiscal cycle in larger samples including these countries. Once these countries are removed from the larger sample, any political fiscal cycle in larger samples disappears.

This finding suggests that fiscal manipulation may “work” because of lack of experience with electoral politics or lack of information that is “produced” in established democracies and that more experienced voters use. As models that view rational voters as “fiscal conservatives” suggest, once a country becomes experienced in electoral politics, a political fiscal cycle should not appear at the macro level.

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Data Appendix

Sample

We use IFS data for all the countries with available central government data on the *Deficit*, *Total Expenditure* and *Total Revenue* (including *Grants*). Where IFS data are missing we tried to complement them by using GFS data or alternative sources. A detailed list of all the adjustments made to the data appears in Table A1.

To restrict our sample only to democracies, we include only the observations with a non-negative score in the *POLITY IV* level of democracy index, which is produced by the University of Maryland. Hence, only data points with a score of 0 and above are left in the sample.

In the former socialist economies in Eastern Europe and the former Soviet Union we exclude the observations for the first two years after transition, as they may represent the simultaneous effect of the shift to democracy and the collapse of central planning, rather than political manipulation of fiscal variables.

Fiscal policy variables

The dependent variables are the following: ***Balance***- calculated as the difference between *Total Revenue & Grants* and *Total Expenditure*. ***Total Expenditure***- taken from the IFS dataset. ***Total Revenue & Grants***- calculated as *Revenue* plus *Grants* from the IFS dataset.

All these variables are presented as a percentage of GDP, the latter also taken from the IFS dataset.

Election variables

The data on election years and dates, are mainly retrieved from the Institute for Democracy and Electoral Assistance (*IDEA*), "Voter Turnout Since 1945 to Date" (www.idea.int/voter_turnout.com). Additional sources are: The International Foundation for Electoral Systems (*IFES*- www.electionguide.org), The Database of Political Institutions (*DPI*) Version 3.0, (a project conducted by the World Bank) and are complemented by other political data sources.

Our election year variable ***ElectWB***- is a dummy variable that receives the value 1 in the election year and 0 otherwise.

All our estimations contain fixed country effects, as well as one lag of the dependent variable. Fixed year effects were tested and removed since they were not statistically significant and have not affected the main results.

Persson & Tabellini economic control variables

Trade- the share of international trade, as a percentage of GDP, taken from the World Development Indicators (WDI) 2002 publication of the World Bank.

Lgdp_pc - The log of real per capita income. The data for 1975-2001 are taken directly from the WDI dataset (mentioned above). The data for the years 1960-1975 are computed using the WDI "GDP per capita in constant 1995 US\$" series.

Pop1564, Pop65 - Two demographic variables measuring the fraction of a country's population, ranging between 15 through 64, and above 65, respectively.

Gdp_rhp - A measure of the output gap, calculated as the difference between real GDP and its (country specific) trend. The trend was computed using the Hodrick-Prescott filter on the change in real GDP. Real GDP data were extracted from the WDI dataset in constant 1995 US\$.

Shi & Svensson control variables

Lgdp_pc- The log of real per capita income – as elaborated above.

Gdp_r - The change in real GDP; taken from the WDI dataset.

Presidential Vs. Parliamentary election rules

The *DPI* database provides information whether the chief executive responsible for economic policy, in each country and in each election year, is elected directly by the public or by parliament. In the former case we define the electoral rule as Presidential and in the latter as Parliamentary, as in Persson and Tabellini (2002). Based on this distinction between the electoral rules we computed the following variables:

Pres - receives the value 1 in a Presidential electoral system, and 0 otherwise.

Parl - receives the value 1 in a Parliamentary electoral system, and 0 otherwise.

Wb_pres - an interaction between *Pres* and *ElectWB*= (*Pres*)*(*ElectWb*).

Wb_parl - an interaction between *Parl* and *ElectWB*= (*Parl*)*(*ElectWb*).

When estimating the Presidential vs. Parliamentary equation, we use both *Wb_pres* and *Wb_parl* variables, together with Persson & Tabellini economic control variables, and one lag of the dependent variable.

Level of democracy

The analysis regarding the level of democracy was based on the score of each country in the *POLITY IV* dataset. We split the sample between these countries with a score of 0 to 9 and those with a score of 10, because more than 50 percent of the data points represent countries with a score of 10.

Table 1: The Political Budget Cycle Across Countries.

		Shi & Svensson's estimation period - without a democracy filter ^{3,4}			Shi & Svensson's estimation period and sample - with a democracy filter			Shi & Svensson's estimation and sample - extended period and democracy filter			Shi & Svensson's estimation for the entire sample and sample period			Persson-Tabellini's control variables ⁵ for the entire sample and sample period		
		(1)			(2)			(3)			(4)			(5)		
Estimation period	Dependent variable ¹	1975-1995			1975-1995			1960-2001			1960-2001			1960-2001		
		balance	texp	trg	balance	texp	trg	balance	texp	trg ⁶	balance	texp	trg	balance	texp	trg
	Electwb²	-0.632*** (0.237)	0.128 (.523)	-0.455*** (0.171)	-0.390** (0.180)	0.119 (0.192)	-0.234 (0.153)	-0.251** (0.127)	-0.051 (0.199)	-0.300* (0.178)	-0.325** (0.128)	0.059 (0.198)	-0.273 (0.173)	-0.329** (0.129)	0.073 (0.199)	-0.255 (0.174)
	Adjusted R²	0.693	0.915	0.950	0.685	0.956	0.966	0.691	0.909	0.914	0.675	0.907	0.916	0.672	0.907	0.917
	F-Statistic	36.989	192.318	333.641	33.351	324.555	430.275	57.007	253.45	270.812	47.964	225.566	252.093	44.967	213.368	240.222
	DW Statistic	1.992	2.010	2.017	1.959	1.945	1.964	1.971	1.529	1.458	1.995	1.596	1.472	1.963	1.582	1.465
	No. of countries	79	79	80	59	59	59	59	59	59	69	69	69	69	69	69
	No. of obs.	1440	1458	1461	924	932	924	1552	1575	1577	1631	1654	1656	1610	1633	1634

¹Variable definitions (all in percent of GDP): **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants

²Electwb - a dummy variable with the value 1 in the election year and 0 otherwise.

³The covariates include one lag of the dependent variable, the log of per-capita GDP, and the change in real GDP during the year.

⁴This equation is based on unadjusted IFS data. For a discussion of the adjustments, see Annex I.

⁵The covariates include one lag of the dependent variable, the log of per-capita GDP, the share of international trade in GDP, the fraction of the population over age 65, the fraction of the population between ages 15 and 64, and the log difference between real GDP and its (country specific) trend, estimated using a Hodrick-Prescott filter.

⁶Coefficient insignificant for sample excluding Sweden. See page 11 of text for a discussion of Swedish data.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 2: The Political Budget Cycle in OECD Economies

Estimation period Dependent variable	OECD economies, using Persson-Tabellini's controls ³			Equation (1), excluding "new democracies" ⁴			OECD economies, using Shi- Svensson's controls ³			Equation (3), excluding "new democracies" ⁴		
	(1)			(2)			(3)			(4)		
	1960-2001			1960-2001			1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg ⁵	balance	texp	trg	balance	texp	trg ⁵
Electwb²	-0.230* (0.123)	-0.103 (0.307)	-0.356 (0.281)	-0.076 (0.124)	-0.166 (0.143)	-0.263** (0.119)	-0.287** (0.119)	-0.059 (0.302)	-0.364 (0.279)	-0.152 (0.118)	-0.043 (0.134)	-0.208* (0.117)
Adjusted R²	0.830	0.868	0.870	0.837	0.971	0.977	0.839	0.864	0.867	0.850	0.974	0.977
F-Statistic	130.906	170.763	180.423	139.427	933.408	1160.376	157.612	194.696	199.812	175.287	1200.348	1343.023
DW Statistic	1.847	1.286	1.237	1.781	1.855	1.854	1.950	1.266	1.216	1.888	1.943	1.860
No. of countries	24	24	24	20	20	20	24	24	24	20	20	20
No. of obs.	800	812	813	703	715	715	811	823	825	714	726	726

¹Variable definitions (all in percent of GDP)**balance**-central government surplus;**texp**-total expenditure by the central government**trg**-total revenue and grants of the central government.

²Electwb - a dummy variable with the value 1 in the election year and 0 otherwise.

³For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

⁴The "new democracies" among the developed economies are Spain, Portugal, Greece and Turkey.

⁵Coefficient insignificant for sample excluding Sweden. See page 11 of text for a discussion of Swedish da

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 3: The Political Budget Cycle in "New Democracies".

Estimation period Dependent variable	All the "new democracies", using Persson-Tabellini's controls ³			All the "new democracies", using Shi-Svensson's controls ³			Equation (1) excluding "transition" economies ⁴			Equation (2) excluding "transition" economies ⁴		
	(1)			(2)			(3)			(4)		
	1960-2001			1960-2001			1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg	balance	texp	trg	balance	texp	trg
Electwb²	-0.853*** (0.298)	0.651** (0.320)	-0.264 (0.238)	-0.793*** (0.295)	0.657** (0.323)	-0.226 (0.245)	-0.641** (0.323)	0.317 (0.315)	-0.387 (0.253)	-0.604* (0.320)	0.350 (0.315)	-0.332 (0.259)
Adjusted R²	0.457	0.930	0.953	0.468	0.929	0.950	0.489	0.919	0.928	0.496	0.919	0.924
F-Statistic	9.562	138.468	208.464	10.610	145.671	209.883	11.401	127.333	140.399	12.758	139.168	145.754
DW Statistic	1.856	2.002	2.079	1.875	2.039	2.099	1.761	1.903	2.077	1.805	1.932	2.148
No. of countries	37	37	37	37	37	37	27	27	27	27	27	27
No. of obs.	438	446	438	438	446	438	359	367	359	359	367	359

¹Variable definitions (all in percent of GDP): **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants of the central government.

²Electwb - a dummy variable with the value 1 in the election year and 0 otherwise.

³For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

⁴The "new democracies" among the transition economies are listed in Table A1.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 4: The Evolution of the "New Democracy" Effect Over Time.

Estimation period Dependent variable ¹	The entire sample, using Persson- Tabellini's controls ³			The entire sample, using Shi-Svensson's controls ³		
	(1)			(2)		
	1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg
Electwb_old ²	-0.075 (0.153)	-0.125 (0.237)	-0.195 (0.206)	-0.105 (0.151)	-0.101 (0.235)	-0.193 (0.205)
Electwb_ND1 ³	-1.169** (0.554)	0.475 (0.836)	-0.773 (0.753)	-0.950* (0.550)	0.061 (0.836)	-1.083 (0.755)
Electwb_ND2 ³	-0.987** (0.392)	0.605 (0.593)	-0.487 (0.533)	-1.025*** (0.390)	0.674 (0.592)	-0.501 (0.534)
Electwb_ND3 ³	-0.962** (0.485)	0.847 (0.752)	0.102 (0.659)	-0.789* (0.462)	0.637 (0.752)	0.012 (0.662)
Electwb_ND4 ³	-1.139* (0.666)	0.081 (1.033)	-0.195 (0.206)	-1.106* (0.661)	0.036 (1.032)	-1.083 (0.908)
Adjusted R²	0.673	0.907	0.916	0.675	0.907	0.916
F- Statistic	42.951	202.405	227.761	45.643	213.451	238.636
DW Statistic	1.962	1.580	1.468	1.991	1.593	1.476
No. of countries	69	69	69	69	69	69
No. of obs.	1610	1633	1634	1631	1654	1656

¹Variable definitions (all in percent of GDP): **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants of the central government.

²Electwb_old - a dummy variable with the value 1 in the election year - only in old democracies - and 0 otherwise.

³Dummy variables with the value of 1 in the election year of the first, second, third and fourth elections, respectively - only in new democracies - and 0 otherwise.

⁴For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 5: The Political Budget Cycle in "Old Democracies".

Estimation period Dependent variable	All the "old democracies", using Persson-Tabellini's controls ³			All the "old democracies", using Shi-Svenssons controls ³			Developing "old democracies", using Persson-Tabellini's controls ³			Developing "old democracies", using Shi- Svenssons controls ³		
	(1)			(2)			(3)			(4)		
	1960-2001			1960-2001			1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg	balance	texp	trg	balance	texp	trg
Electwb²	-0.062 (0.136)	-0.143 (0.150)	-0.203 (0.124)	-0.100 (0.133)	-0.085 (0.146)	-0.175 (0.121)	-0.040 (0.320)	-0.101 (0.351)	-0.054 (0.283)	-0.079 (0.312)	-0.077 (0.343)	-0.048 (0.278)
Adjusted R²	0.769	0.961	0.969	0.771	0.963	0.969	0.695	0.945	0.952	0.695	0.947	0.953
F-Statistic	96.202	713.969	908.312	107.225	826.264	1026.498	49.454	368.802	432.427	60.686	474.078	551.248
DW Statistic	1.900	1.995	1.866	1.932	2.053	1.892	1.973	2.123	1.884	1.976	2.148	1.920
No. of countries	32	32	32	32	32	32	12	12	12	12	12	12
No. of obs.	1086	1101	1109	1107	1122	1130	383	386	394	393	396	404

¹Variable definitions (all in percent of GDP): **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants of the central government.

²Electwb - a dummy variable with the value 1 in the election year and 0 otherwise.

³For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 6: The Effect of the Type of Electoral System on the Political Budget Cycle

Estimation period Dependent variable ¹	All countries, using Persson-Tabellini's controls ⁴			"New democracies", including the "transition" economies ⁵ , using Persson-Tabellini's controls ⁴			"Old democracies" using Persson-Tabellini's controls ⁴		
	(1)			(2)			(3)		
	1960-2001			1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg	balance	texp	trg
Wb_pres²	-0.468** (0.232)	0.447 (0.355)	0.036 (0.311)	-0.803** (0.360)	0.634 (0.391)	-0.192 (0.288)	0.017 (0.328)	-0.011 (0.356)	0.101 (0.292)
Wb_parl³	-0.267* (0.154)	-0.092 (0.238)	-0.385* (0.209)	-0.957* (0.518)	0.684 (0.541)	-0.414 (0.412)	-0.078 (0.148)	-0.171 (0.165)	-0.267** (0.135)
Adjusted R²	0.672	0.907	0.917	0.456	0.930	0.953	0.769	0.961	0.969
F-Statistic	44.368	210.664	237.122	9.323	134.987	203.316	93.654	695.120	885.314
DW Statistic	1.962	1.581	1.466	1.854	2.001	2.079	1.902	1.993	1.868
No. of countries	69	69	69	37	37	37	32	32	32
No. of obs.	1610	1633	1634	438	446	438	1086	1101	1109

¹Variable definitions (all in percent of GDP): **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants of the central government.

²WB_pres - a dummy variable with the value 1 in the election year, if the regime is presidential, and 0 otherwise.

³WB_parl - a dummy variable with the value 1 in the election year, if the regime is parliamentary, and 0 otherwise.

⁴For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

⁵The "new democracies" among the transition economies are listed in Table A1.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 7a: The Effect of the Level of Democracy on the Political Budget Cycle - All Countries

Estimation period Dependent variable ¹	All countries with level of democracy between 0 to 9, using Persson-Tabellini's controls ³			All countries with level of democracy between 0 to 9, using Shi-Svensson's controls ³			All countries with level of democracy of 10, using Persson-Tabellini's controls ³			All countries with level of democracy of 10, using Shi-Svensson's controls ³		
	(1)			(2)			(3)			(4)		
	1960-2001			1960-2001			1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg	balance	texp	trg	balance	texp	trg
Electwb²	-0.599** (0.249)	0.411 (0.267)	-0.170 (0.203)	-0.578** (0.245)	0.390 (0.265)	-0.188 (0.204)	-0.125 (0.120)	-0.124 (0.288)	-0.264 (0.269)	-0.174 (0.117)	-0.061 (0.284)	-0.243 (0.266)
Adjusted R²	0.535	0.937	0.953	0.538	0.937	0.953	0.822	0.865	0.861	0.829	0.866	0.860
F-Statistic	18.204	224.990	315.030	19.831	244.470	336.039	104.035	146.447	139.185	119.873	162.309	151.542
DW Statistic	1.989	2.141	1.995	1.989	2.156	2.021	1.836	1.310	1.281	1.897	1.305	1.273
No. of countries	43	43	43	43	43	43	33	33	33	33	33	33
No. of obs.	735	743	758	744	752	767	872	887	873	884	899	886

¹Variable definitions (all in percent of GDP) **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants of the central government.

²Electwb - a dummy variable with the value 1 in the election year and 0 otherwise.

³For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 7b: The Effect of the Level of Democracy on the Political Budget Cycle - "New Democracies"

Estimation period Dependent variable ¹	"New democracies" with level of democracy between 0 to 9, using Persson-Tabellini's controls ³			"New democracies" with level of democracy between 0 to 9, using Shi-Svensson's controls ³			"New democracies" with level of democracy of 10, using Persson-Tabellini's controls ³			"New democracies" with level of democracy of 10, using Shi-Svensson's controls ³		
	(1)			(2)			(3)			(4)		
	1960-2001			1960-2001			1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg	balance	texp	trg	balance	texp	trg
Electwb²	-0.802** (0.340)	0.502 (0.362)	-0.383 (0.269)	-0.704** (0.338)	0.516 (0.366)	-0.316 (0.278)	-1.101** (0.455)	1.546** (0.633)	0.539 (0.476)	-1.109** (0.446)	1.679*** (0.619)	0.616 (0.470)
Adjusted R²	0.410	0.912	0.940	0.421	0.910	0.936	0.810	0.952	0.969	0.816	0.954	0.970
F-Statistic	7.587	100.909	150.278	8.473	106.641	150.740	18.911	83.860	134.947	24.755	111.965	175.152
DW Statistic	1.861	1.987	2.010	1.881	2.045	2.070	1.947	1.645	2.246	2.069	1.885	2.307
No. of countries	33	33	33	33	33	33	8	8	8	8	8	8
No. of obs.	371	379	371	371	379	371	60	60	60	60	60	60

¹Variable definitions (all in percent of GDP) **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants of the central government.

²Electwb - a dummy variable with the value 1 in the election year and 0 otherwise.

³For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table 7c: The Effect of the Level of Democracy on the Political Budget Cycle - "Old Democracies"

Estimation period Dependent variable ¹	"Old democracies", with level of democracy between 0 to 9, using Persson- Tabellini's controls ³			"Old democracies", with level of democracy between 0 to 9, using Shi-Svensson's controls ⁴			"Old democracies", with level of democracy of 10, using Persson- Tabellini's controls ³			"Old democracies", with level of democracy of 10, using Shi-Svensson's controls ⁴		
	(1)			(2)			(3)			(4)		
	1960-2001			1960-2001			1960-2001			1960-2001		
	balance	texp	trg	balance	texp	trg	balance	texp	trg ⁴	balance	texp	trg
Electwb²	-0.265 (0.389)	0.005 (0.428)	-0.156 (0.324)	-0.289 (0.379)	0.035 (0.415)	-0.134 (0.315)	-0.026 (0.124)	-0.160 (0.141)	-0.201* (0.120)	-0.071 (0.121)	-0.073 (0.135)	-0.154 (0.118)
Adjusted R²	0.666	0.952	0.963	0.665	0.955	0.965	0.823	0.967	0.972	0.830	0.970	0.972
F- Statistic	37.865	369.663	525.110	47.549	493.971	693.880	118.571	765.526	877.671	139.584	921.939	987.852
DW Statistic	1.947	2.157	1.771	1.965	2.146	1.788	1.834	1.928	1.973	1.880	1.986	1.989
No. of countries	10	10	10	10	10	10	25	25	25	25	25	25
No. of obs.	297	297	320	306	306	329	784	800	785	796	812	797

¹Variable definitions (all in percent of GDP): **balance**-central government surplus; **texp**-total expenditure by the central government; **trg**-total revenue and grants of the central government.

²Electwb - a dummy variable with the value 1 in the election year and 0 otherwise.

³For the list of covariates in the Persson-Tabellini and Shi-Svensson specifications, see Table 1.

⁴Coefficient insignificant for sample excluding Sweden. See page 11 of text for a discussion of Swedish data.

*-Significant at the 10 percent level; **-Significant at the 5 percent level; ***-Significant at the 1 percent level.

Table A1: Sample Characteristics and data adjustments

No.	Country	Years with positive Polity value	Years with available data	Years not using IFS - and description of the adjustments to the fiscal data	OECD economy	"New democracy"	Other comments	Number of elections in the sample ¹
1	Argentina	1973-75, 1983-2001	1983-2000	IFS data for years before 1995 are missing; GFS data were used. Since GFS data are presented as an index, 1978 was used as a base year,		x	Fiscal data not available for 1973-1975. The change in real GDP for 1991 and 1992 is calculated from IFS 2003.	3
2	Australia	1960-2001	1960-2000	1998-99 GFS, 2000 IMF staff report	x		The change in real GDP for 1968 is calculated from IFS.	17
3	Austria	1960-2001	1960-99	1999 - GFS. There are breaks in the IFS series in 1970, 1980 and 1990. These were bridged by using differences from OECD data.	x			13
4	Belgium	1960-2001	1960-98	There is a break in the Series in 1970. It was bridged by using the difference from the OECD dataset	x			13
5	Bolivia	1982-2001	1986-2000	1988-1991 - GFS. 1982-1984 are excluded because the revenue data are not consistent with the following years				3
6	Brazil	1960-63, 1985-2001	1988-98	1998 - GFS. Data for 1995-96 missing		x		3
7	Bulgaria	1990-2001	1990-2000	1990-1991 excluded, transition years.		x		2
8	Canada	1960-2001	1960-2000				Real GDP and population data for 1960-1965 and trade data for 1960-64 and 2000, calculated from IFS (the 2000 data from IFS 2003).	13
9	Chile	1960-72, 1989-2001	1971-72, 1989-2001	2001, IMF staff report. Data prior to 1971 are unreliable and excluded. 1971-72 excluded because of missing lags.	x			2
10	Colombia	1960-2001	1971-93	Expenditure data: 1971-1993 - GFS (IFS data not available).		x		10
11	Costa-Rica	1960-2001	1972-99	Revenue: 1972-1999 - GFS; IFS revenue data are not comparable to the expenditure data.				7
12	Cyprus	1960-62, 1968-2001	1975-2001	2001, IMF staff report			The Greek part of Cyprus.	5
13	Czech Republic	1990-2001	1993-2000			x		2
14	Denmark	1960-2000	1960-99	A break in the series in 1970 was bridged using the differences calculated from OECD data.	x			15
15	Dominican Republic	1963, 1978-2001	1978-2000					3
16	Ecuador	1960, 1968-71, 1979-2001	1979-2001			x		3
17	El Salvador	1964-70, 1984-2001	1984-2000			x		3+1
18	Estonia	1991-2001	1991-2000	1991-1992 excluded - transition years.		x		2
19	Fiji	1970-86, 1990-99	1970-86, 1990-99	A gap in the IFS data in 1998 was bridged using differences from the IMF staff report data.		x		4+1

No.	Country	Years with positive Polity value	Years with available data	Years not using IFS - and description of the adjustments to the fiscal data	OECD economy	"New democracy"	Other comments	Number of elections in the sample ¹
20	Finland	1960-2001	1960-98	Missing revenue data for 1968-1971 were bridged by using differences from OECD data.	x			11
21	France	1960-2001	1972-97		x			10
22	Germany ⁸	1960-2001	1960-98	A break in 1970 was bridged using differences from OECD data.			West Germany until 1990. GDP data prior to 1972 were calculated from IFS 2003. Trade data prior to 1972 are not available.	11
23	Greece	1960-66, 1975-2001	1960-66, 1975-98	1994-1998 - GFS, due to extra-ordinary expenditure data reflecting accounting adjustments. GDP was revised in 1988: GDP for 1975-1987 was multiplied by 1.23 to be consistent with the revised level. The 1982 expenditure figure was corrected using GFS.	x			4+5
24	Guatemala	1966-73, 1986-2001	1966-73, 1986-2000		x	x		4+1
25	Guyana	1966-79, 1992-2001	1966-79, 1992-97			x		3
26	Honduras	1982-2001	1990-2000			x		4
27	Hungary	1990-2001	1990-2000			x		2
28	Iceland	1960-2001	1972-2000				Trade data for 2000 were calculated from IFS 2003.	8
29	India	1960-2001	1960-2000		x			10
30	Ireland	1960-2001	1960-99		x			11
31	Israel	1960-2001	1960-2001	2000-2001, using BOI data. 1973, 1985 excluded due to war and hyper-inflation, respectively. The 1991 budget figures are multiplied by 1.33 to account for the 9 month fiscal year.				10
32	Italy	1960-2001	1960-98		x			10
33	Japan	1960-2001	1970-93	IFS data not available. GFS was used.	x			8
34	Korea	1960, 1963- 71, 1988- 2001	1963-71, 1988-97			x		2+1
35	Lithuania	1991-2001	1993-2000			x		2
36	Luxembourg	1960-2001	1970-74, 1976-97	1975 - a break in the series. 1970-72 GFS data.	x			5
37	Madagascar	1992-2001	1992-2000			x		2
38	Malaysia	1960-2001	1960-99					9
39	Mali	1992-2001	1992-2000					1
40	Mauritius	1968-2001	1968-2000			x		
41	Mexico	1988-2001	1988-2000				Democratic elections took place since 1958 - before independence.	8
42	Nepal	1990-2001	1990-99			x		2
						x		4

No.	Country	Years with positive Polity value	Years with available data	Years not using IFS - and description of the adjustments to the fiscal data	OECD economy	"New democracy"	Other comments	Number of elections in the sample ¹
43	Netherlands	1960-2001	1960-98		x			12
44	New Zealand	1960-2001	1960-2000	1989 - GFS			Trade data for 1960-72 and 2000 are taken from IFS 2003.	13
45	Nicaragua	1990-2001	1990-2001				GDP and trade data were calculated from IFS 2003.	2
46	Norway	1960-2001	1960-98	A break in the fiscal series in 1972 is bridged by using differences from OECD data.		x		10
47	Pakistan	1962-68, 1973-76, 1988-98	1973-76, 1988-98		x		The period before 1973 was excluded because Pakistan included Bangladesh.	3
48	Panama	1960-67, 1989-2001	1960-67, 1989-2000			x	Trade data for 1960-67 are taken from IFS 2003.	2+1
49	Papua New Guinea	1975-2001	1975-99			x	Elections took place before complete independence in 1975.	6
50	Paraguay	1989-2001	1989-2001			x	GDP and trade data were calculated from IFS 2003.	2
51	Peru	1960-67, 1980-91, 1993-99	1986-99			x	1992 is included - despite a negative Polity grade - to avoid a break in the series.	2
52	Philippines	1960-71, 1987-2001	1960-71, 1987-2001			x		3+3
53	Poland	1989-2001	1991-2000	Fiscal data for 1991-1993 were calculated on the basis of differences from OECD data.		x		2
54	Portugal	1976-2001	1976-98					4+2
55	Romania	1990-2001	1990-99		x	x		2
56	Russia	1992-2001	1995-2000			x		2
57	Slovak Republic	1993-2001	1994-2000	1994 and 1995 data were calculated by using differences from OECD data.		x		1
58	Slovenia	1991-2001	1993-2001			x		1
59	South Africa	1960-91, 1994-2001	1960-91, 1994-2000			x		8
60	Spain	1978-2001	1978-2000	1999 and 2000 were calculated by using differences from OECD data.				4+3
61	Sri Lanka	1960-2001	1960-2000		x	x		7
62	Sweden	1960-2001	1960-2000				There are substantial breaks in the series in the early 1990s.	12
63	Switzerland	1960-2001	1960-2000		x		Trade data for 2000 were calculated from IFS 2003	10

No.	Country	Years with positive Polity value	Years with available data	Years not using IFS - and description of the adjustments to the fiscal data	OECD economy	"New democracy"	Other comments	Number of elections in the sample¹
64	Trinidad & Tobago	1962-2001	1962-72, 1976-89, 1993-95					6
65	Turkey	1961-70, 1973-79, 1983-2001	1983-2000				Previous periods excluded due to lack of data and shortness of sample period.	3+2
66	United Kindom	1960-2001	1960-2000	Fiscal data for 2000 were calculated using differences from OECD data	x	x		9
67	United States	1960-2001	1960-2000		x		Trade data for 2000 were calculated from IFS 2003	10
68	Uruguay	1960-70, 1985-2001	1985-2000					3
69	Venezuela	1960-2001	1960-2000			x		8

¹The figure after a + sign indicates the number of elections that took place in a country which is defined as a "new democracy" during years in which it was not a "new democracy".

Table A2: The "New Democracies"

No.	Country	Year of Becoming a Democracy ¹	Elections Included as a "New Democracy"	"Transition" Economy
1	Argentina	1983	89, 95, 99	
2	Bolivia	1982	89, 93, 97	
3	Brazil	1985	89, 94, 98	
4	Bulgaria	1990	92, 96	x
5	Chile	1989	93, 00	x
6	Czech Republic	1990	96, 98	
7	Dominican Republic	1978	82, 86, 90	
8	Ecuador	1979	84, 88, 92	
9	El Salvador	1984	89, 94, 99	
10	Estonia	1991	95, 99	x
11	Fiji	1970	73, 77, 82, 92	
12	Greece	1975	77, 81, 85, 89	
13	Guatemala	1966, 1986	70, 90, 95, 99	
14	Guyana	1966, 1992	68, 73, 97	
15	Honduras	1982	85, 89, 93, 97	
16	Hungary	1990	94, 98	x
17	Korea	1988	92, 97	
18	Lithuania	1991	93, 97	x
19	Madagascar	1992	93, 96	
20	Mali	1992	97	
21	Mexico	1988	94, 00	
22	Nepal	1990	91, 94, 97, 99	
23	Nicaragua	1990	90, 96	
24	Pakistan	1988	91, 94, 97	
25	Panama	1989	94, 99	
26	Paraguay	1989	93, 98	
27	Peru	1980	90, 95	
28	Philippines	1987	92, 95, 98	
29	Poland	1989	95, 00	x
30	Portugal	1976	80, 83, 85	
31	Romania	1990	92, 96, 00	x
32	Russia	1992	96, 00	x
33	Slovak Republic	1993	98	x
34	Slovenia	1991	97	x
35	Spain	1978	79, 82, 86, 89	
36	Turkey	1983	87, 91, 95	
37	Uruguay	1985	89, 94, 99	

¹The first year in which the country receives a positive value in the POLITY scale, following a substantial period of negative values. The actual transition (e.g., first democratic elections) can take place during the previous year.

Source: Calculations based on the POLITY IV dataset, produced by the University of Maryland, and the World Bank Database on Political Institutions.

Table A3: Countries excluded from the sample¹

No.	Country	Years With Positive Polity	Reason for exclusion
1	Bahamas, The	no polity	No POLITY rank
2	Bangladesh	72-73,91-2001	No fiscal data in IFS.
3	Barbados	no polity	No POLITY rank
4	Belarus	1991-1995	Available sample too short.
5	Belize	no polity	No POLITY rank
6	Bostwana	1966-2001	Extra-ordinary changes in the series.
7	Burkina Faso	78-79	Only two years with positive POLITY rank
8	Burundi	all negative	Negative POLITY rank throuout the sample period.
9	Cameroon	all negative	Negative POLITY rank throuout the sample period.
10	Chad	all negative	Negative POLITY rank throuout the sample period.
11	Congo	60-62,92-96	Sample too short
12	Croatia	2000	Sample too short
13	Egypt, Arab Rep.	all negative	Negative POLITY rank throuout the sample period.
14	Gambia, The	65-93	No fiscal data in IFS.
15	Ghana	70-71,79-80,96-2001	No fiscal data in IFS.
16	Indonesia	1999-2001	Sample too short
17	Iran	1997-2001	Sample too short
18	Jamaica	1960-2001	No fiscal data in IFS.
19	Kenya	1963-1968	Sample too short
20	Latvia	1991-2001	Sample too short
21	Liberia	1997-2001	Sample too short
22	Malawi	1994-2001	Sample with IFS data too short
23	Maldives	no polity	No POLITY rank
24	Malta	no polity	No POLITY rank
25	Nigeria	60-65,79-83,99-2001	Each democratic episode is too short.
26	Senegal	2000-2001	Sample too short
27	Siera Leone	61-66,68-70,97	Each democratic episode is too short.
28	Singapore	1960-1962	Sample too short
29	Solomon Islands	no polity	No POLITY rank
30	St.Lucia	no polity	No POLITY rank
31	Suriname	no polity	No POLITY rank
32	Syrian, Arab Rep	all negative	Negative POLITY rank throuout the sample period.
33	Thialand	69-70,74-75,78-90,92-2001	Too many breaks in the periods of democracy.
34	Togo	all negative	Negative POLITY rank throuout the sample period.
35	Tunisia	all negative	Negative POLITY rank throuout the sample period.
36	Zambia	64-71,91-2001	Extra-ordinary changes in the series.
37	Zimbabwe	70-78,80-86	Available periods too short.

¹Countries that appear in the IFS or that were used in other studies.